Editorial

J Gynecol Oncol Vol. 25, No. 3:162-163
http://dx.doi.org/10.3802/jgo.2014.25.3.162
pISSN 2005-0380 · eISSN 2005-0399

Journal of Gynecologic Oncology
jgo

Shall we settle for low-level evidence?

Mansoor Raza Mirza
Department of Oncology, Rigshospitalet, Copenhagen University Hospital, Copenhagen, Denmark

See accompanying article by Narayan and colleagues on page 206.

Cervical cancer is only moderately sensitive to radiation therapy and satisfactory tumor control requires fairly high delivered dose. Historically this has been achieved by combination of external-beam radiotherapy (EBRT) with intracavitary brachytherapy. While dose distribution of EBRT is fairly homogenous, inverse-square law determines most of the dose distribution in brachytherapy. The dilemma has been to deliver as high dose as possible to the target (tumor) sparing adjacent organs at risk. Earlier published data by teams in Vienna and Paris has resulted in the guidelines for conformal brachytherapy.

In this issue Narayan et al. [1] describes their long-term experience of delivering conformal brachytherapy with the assistance of much simpler technique, transabdominal ultrasound, than prescribed by other authors. Transabdominal ultrasound is widely available and other centres can easily adapt this method. The reported efficacy of local control and toxicity profile is comparable to previously reported results by other authors. Sample size in this report is fairly large and patients are treated quite homogenously over the years with a few changes in brachytherapy fractionation. Posttreatment magnetic resonance imaging (MRI) elegantly performs quality control of this study.

All in all it is exciting that another modality is now available for image guided conformal brachytherapy. The described modality is effective, practical and perhaps cheaper than earlier reported techniques. For all above I would like to congratulate the authors. Though as Bertrand Russell said, "in all affairs it's a healthy thing now and then to hang a question mark on the things you have long taken for granted."

Below are a few thoughts, which I feel are left unanswered:

(1) Is this evidence sufficient to change our practice? This is a retrospective analysis of a single institution. In medical oncology such results will only be used as hypothesis generating and will never be accepted as practice changing by physicians or accepted by the regulatory authorities like European Medicines Agency (EMA) and US Food and Drug Administration (FDA). Unfortunately authors does not describe if they plan to validate their results through a randomized prospective trial. I read their conclusions as, "this is it, this is the state of the art." I strongly recommend authors to continue their work to achieve level-one evidence in this field.

(2) Can we further improve local control? Patterns of failure at local site are not addressed. They describe the number of central relapses and that the frequency of these relapses is comparable to earlier reported series, though I miss the information of where exactly relapse occurred—at the rim of the tumor where dose delivery was rather low or at the most bulky site of the disease? The positron emission tomography-computed tomography (PET-CT) or CT was performed in the follow-up period and these data can possibly be extracted. This information from relapse patients will let authors learn and could eventually modify the treatment to achieve even better local control. One possibility can be to modify target dose (higher EBRT dose to some area of tumor) by performing dose-painting treatment plans.

(3) Is it appropriate to discuss about survival while reporting isolated results of a fraction of therapy? Authors compare the overall survival with other studies and conclude that the results are comparable. First they do not mention that there is only a fraction of increment on survival by brachytherapy. The multi-modality treatment of cervical cancer has positively influenced on survival. There is a consensus that survival increment of EBRT and brachytherapy has long reached to saturation and that improvement in systemic cytotoxic therapy or targeted therapy is needed in order to improve survival of these patients. Secondly comparison of single

Correspondence to Mansoor Raza Mirza
Department of Oncology, Rigshospitalet, Copenhagen University Hospital, Blegdamsvej 9, 2100 Copenhagen, Denmark. E-mail: mansoor.raza.mirza@regionh.dk

centre retrospective studies can be misleading due to the in-built bias of these reports.

(4) Can we draw any conclusions from uncontrolled data collection of a retrospective analysis? Authors describe that the toxicity profile in their cohort study is comparable to previously reported studies. One would have hoped that delivering lesser dose might cause lesser toxicity. Authors did not discuss the weakness of retrospective extraction of toxicity data from patient journals. There is no standard toxicity reporting protocol in this study.

I would conclude that this large single-centre cohort results are well described within the limits of the bias due to lack of prospective design and due to lack of a randomized comparator. The results of this analysis are encouraging, though only hypothesis generating and there is a need for validation through a prospective randomized trial to acquire level-one evidence, before the community can adapt this technique. We owe our patients not to settle for low-level evidence!

CONFLICT OF INTEREST

No potential conflict of interest relevant to this article was reported.

REFERENCE

1. Narayan K, van Dyk S, Bernshaw D, Khaw P, Mileshkin L, Kondalsamy-Chennakesavan S. Ultrasound guided conformal brachytherapy of cervix cancer: survival, patterns of failure, and late complications. J Gynecol Oncol 2014;25:206-13.